

marking distinctly the commencement and simultaneousness of the storm.

The disturbance in the vertical force commenced about May 14, 1h. 10m. A.M. (7h. 8m. G. T.), and terminated about 3h. 30m. A.M. Between this time and 5h. 45m. P.M. the trace was smooth, but between 5h. 45m. and 6h. 45m. a sharp deflection took place in the opposite direction, the maximum force occurring at 6h. 11m. P.M. (May 15, oh. 9m. A.M. G. T.); the deflection or increase amounted to  $\frac{1}{3}\frac{1}{2}$  of the vertical force.

The vertical force trace did not exhibit any of the tremulous motion noticed in the two other curves.

I may state that a description of the Madison Magnetic Observatory, together with the discussion of the first year's observations and results, is nearly ready in MS. for publication.

Coast Survey Office,  
Washington, D.C.,  
January 6

CHARLES A. SCHOTT,  
Assistant, C. and G. S., in  
charge of Observatory

### Migration of Birds—A Suggestion

I BELIEVE that the migration of birds is one of the most interesting problems to the many who dabble in the narrative branch of natural history. It must also be of some interest to those who study biology in its more strictly scientific form. And yet there appears to be but little accurate information on the subject; nor, as far as I can discover, have any attempts been made to collect careful simultaneous observations over a wide extent of country, which would appear to be the readiest, if not the only means of collecting reliable data.

I am therefore induced to suggest that you should devote a small space in NATURE weekly, at certain seasons of the year, to the record of observations made in all parts of the world, from which correspondents can be induced to send them, of the passage of any migratory birds.

The record should, I think, be in a tabular form, giving the place, the date of appearance or disappearance of any migratory bird, the name of the observer, and some few remarks that may appear pertinent, such as the state of the weather, the direction of the wind, the character of the night, whether dark or moonlight, in the case of birds which are supposed to travel at that time. The necessity for recording other facts will no doubt be suggested by those more conversant with the subject. From these suggestions and the experience gained in a short time, detailed instructions might with advantage be framed for observers. The observers should be careful to state whether they were in a position to know with certainty that the date given was that on which the birds really arrived or left, or whether it was the first time their attention had been called to the circumstance.

I should imagine you would soon have a large number of careful observers interested in the subject not only in the country but abroad also. Among these some means should be taken to enlist captains of ships, or other seafaring men. There appear to be few, if any, recorded observations of birds of passage being seen on the wing at sea—though I may be wrong in this, for I do not profess to have gone carefully into the subject. Still, it is curious that one does not hear oftener of flights of such birds as the woodcock, millions of which come to and leave this country every year, being seen at sea, over long stretches of which they are believed to fly.

J. F. D. DONNELLY

### The Formation of Mountains

THE letter of the Rev. O. Fisher in NATURE, vol. xix. p. 266, is conclusive as to the more rapid cooling of the interior than the outer crust of a heated globe under the conditions of our earth, and I thank him for clearing up the point. But the question remains, whether the amount of contraction of the interior, and consequent crumpling of the crust, thereby produced in a definite time, is sufficient to account for the elevation of our mountains. It is necessary to take account of the following facts:—

1. That the greater part of the elevation of all our chief mountain ranges occurred during the eocene and miocene periods.

2. The warmer climates of those periods (certainly due to external and not to internal heat) would have tended to diminish the rate of cooling and consequent contraction of the earth.

3. The Rev. O. Fisher appears to have demonstrated that, even allowing for the total shrinkage due to the earth's cooling for the last hundred million years (from a mean temperature of 7,000° F.,

as calculated by Sir William Thomson), the amount of elevation thereby caused would be *very much* less than that of existing lands and mountains. But we know that these have been lowered by denudation, and again elevated many times over during that period.

The inadequacy of the alleged cause for the production of our existing mountains would therefore seem to be conclusively established.

ALFRED R. WALLACE

MR. WALLACE's letters raise three separate difficulties: How can the interior of the earth be cooling faster than its surface? How can this surface be cooling so slowly (or not cooling at all) as it is assumed to be? and, How can the different rates of cooling of interior and surface account for the corrugations observed in the surface?

As to the first question, Mr. Fisher's explanations should remove the difficulty.

As to the second question. So long as the amount of heat radiated from the surface into space equals the amount received by the surface from the sun and from the interior together, its temperature cannot fall. The rate of radiation depends on the difference between the temperatures of the surface and space. The supply of heat from the interior depends on the difference between the temperatures of the interior and the surface. Since the temperature of the interior is falling, the supply of heat from interior to surface must diminish. Hence the temperature of the surface must also fall. We cannot doubt that it does, though at present imperceptibly. The extreme slowness is due partly to the fact that the difference of temperature between interior and surface must many times exceed that between the surface and space, even if the latter be at absolute zero; partly to the low transmissive powers both of earth and atmosphere; partly to the immense mass of the earth, from which immense quantities of heat must pass away to lower the temperature of the interior but a trifle.

As to the third question, whether this small fall in temperature will account for observed corrugations, I will offer no opinion. The size of the earth must be allowed for. A diminution of a foot in radius would diminish the surface by more than nineteen square miles.

I do not understand why corrugations should be confined to the surface. I should expect them to extend as deep as the solid crust. They are possible in any stratum which is contracting more slowly than interior strata.

E. HILL

St. John's College, Cambridge

### Bees' Stings

CAN any of your readers inform me why the working honey-bee has such an imperfect weapon of defence as its sting manifestly is? For purposes of self-defence it is apparently worse than useless, for in nearly every case, almost without exception, the bee lays down its life with the sting. The possession of a sting therefore only leads to its own destruction instead of to its preservation so far as the individual bee is concerned. No doubt the hive generally gains an advantage from all its active members having stings and so indirectly do individual bees from the fact that the welfare of the hive, speaking generally, means the welfare of the individuals that compose it. Directly, however, the possession of a sting can only be a disadvantage to the individual bee, unless there are certain enemies from which bees after inflicting a wound can withdraw their stings and escape with life. This so far as my observations go appears to be very unlikely, and therefore no bee can have any knowledge from experience of what a weapon of offence he possesses for he has never used it, nor can he have knowledge from experience of the consequence of using it. All smaller pests bees attack with their jaws. Is it possible, then, that they are so intelligent as to be well aware of the power for mischief to themselves as well as to others which they carry about with them, and that it is only when they altogether lose control over themselves, either through severe pain or through terror lest their queen should be injured that they sign their own death-warrants on our hands and faces? In the death of a few worker-bees a hive suffers very little loss, perhaps none at all; yet it may have gained much in the shape of security from molestation. Are bees so intelligent as to know this fact and communicate it from one to another, or can their conduct be explained on the lower ground of instinct?

It seems that an interesting point is here raised which perhaps

has been fully discussed elsewhere without my knowledge of it. Is the fact that the sting of the worker-bee is an imperfect weapon of defence, a result of its having nothing to do with the propagation of its species, this being left to the stingless queen and drones? Consequently any tendency to develop a more effective sting in one generation of worker-bees has no hereditary effect on succeeding generations, nor apparently have the worker-bees any influence whatever on the worker-bees that succeed them, except by the way in which they feed and educate them, unless, indeed, they can impress their tendencies on the drones or on the future queen before she leaves the hive. If they have no such power, it seems likely that they will always have to lament the use of a weapon which nature might have made as effective as the sting of a wasp. Finally, are there any other insects in the same predicament as worker-bees, *i.e.*, unable to use their weapons of defence without doing themselves more injury than they inflict on their adversary, and unable to help their successors by the transmission of a continually accumulating instinct?

Manningtree, January 22

### Molecular Vibrations

MR. CHAPPELL is certainly right in stating that "the noises in a belfry are most discordant." He might have said (what no doubt he meant) that the sounds emitted by each single bell are most discordant. Every bell which is at all tolerable, possesses, it is true, one predominating note due to the thick part of the bow, where the clapper strikes, but there are also innumerable other notes, some of which *may* be harmonics, while the majority are not so at all. This is presumably often owing to flaws and other defects in casting, but there is another cause common to every case, which is due to the following fact:—

All bells are cast of a conventional shape, with varying diameters from bow to crown. Now every part of a bell, taken vertically, comes into vibration when struck, and in order to give a true note, each horizontal section ought to have a certain exact thickness of metal proportional to its diameter. This is easily verified to the ear by tapping the bell gently at all parts from the bow upwards. Every inch gives a different rate of vibration, and, consequently, a different pitch.

About the time when the second "Big Ben" was cast, which is a long time ago, I tried experimentally to ascertain what the law was which regulated the thickness of the metal in relation to the diameter of the bell, so that every section might be of identical pitch. This was done by casting a series of bell metal rings of varying diameters, and tuning them, by turning in a lathe, to exact unison. So far as my recollection now serves me, the following was the result:—

Measuring all the rings by their outside diameters, no un-deviating rule was apparent, and the same was the case when the inside diameters were compared. When, however, a circle was taken whose circumference was, as nearly as possible, one-third from the outside of the thickness and two-thirds from the inside, then the law came out distinctly that the thickness of the metal must be proportionate to the square of the diameter of such circle. It occurred at once that this circle must, in fact, constitute the neutral axis of vibration. Working on this principle, it seemed worth while to try whether a bell could not be constructed free from discordant sounds. I may shortly say that this proved to be possible, but only by turning the actual casting with great care and accuracy in a lathe. It became evident that the slightest variation in the true thickness vitiated the unisonal character of the tone. A "miss was as good (or as bad) as a mile," and consequently the process of casting itself was too rough for obtaining the desired end.

It may fairly be gathered from Mr. Chappell's letter that he is not enamoured of a "triple bob major," and that he does not class bells generally as musical instruments. I am much afraid he never will. If the present shape and mode of construction (and let me add, the present mode of change ringing) is adhered to, a peal of bells which will quite satisfy a musical ear may be regarded as a practical impossibility.

R. H.

### Missing Nebulæ

IN the note on missing nebulae in NATURE, vol. xix. p. 221, I find the nebulae G. C. 132, 4570, and 5051 mentioned together with the Merope nebula as being diffused objects which are "overlooked in very large telescopes, though obvious in much smaller ones." This alludes, no doubt, to the occurrence of

these objects in the list of nebulae not found with Lord Rosse's 6-foot reflector (*Phil. Trans.*, 1861, p. 745).

With regard to the first object, G. C. 132, it has only been looked for once at Birr Castle, and in the N.P.D. 111° 30' it is possible to account for its non-appearance either by a tilting of the speculum or by the haziness of the sky in this low altitude. G. C. 4570 has been seen three times, and only twice searched for in vain, both times in twilight. G. C. 5051 was set for twice and not found, but 15° north of the zenith the tilting of the speculum almost always changes the index-error of the setting-circle considerably, as expressly stated by the observer on one of the two occasions alluded to. The Merope nebula was last winter seen very distinctly, and roughly sketched with a low power and large field.

J. L. E. DREYER

The Observatory, Dunsink, Co. Dublin, January 13

### Time and Longitude

Now that mankind begin to have settlements, even continental, as appears from Mr. Latimer Clarke's account of Sitka, subject to the inconvenience that he and Mr. Layard point out, is it not time that we agreed to make the line dividing "yesterday from to-morrow" avoid all continents, by taking advantage of two very convenient, if not providential, facts, which are certain, though each was *a priori* highly improbable? First, there were great chances against a globe with our existing proportion of land to water, of coast-lines to area, and of large and small lands to each other, having any Behring Strait, admitting one degree of longitude, or thereabouts, to enjoy the above property. But next, there was still greater chance, perhaps, against the exact opposite degree to the strait covering several national observatories; not only more of them, I think, than any equally narrow meridional band, but the only one that, on historical grounds, we can conceive distant civilised nations accepting without jealousy as a common centre. The antimeridians of Copenhagen, Uraniburg, Leipzig, Munich, Padua, Venice, and Florence, seem to avoid both continents; possibly also those of Christiania, Gotha, Verona, and Modena. Those of Berlin, Prague, Naples, and Palermo, seem a very few miles too far east. Europe proper, and its present railways, are very closely bisected by this street of observatories; the local time of the furthest points each way varying but an hour and a half from it. But the chief coincidence is yet unnamed. Would the pride of any existing land, except China, refuse to make a standard meridian of Rome?

The very Chinese must allow Europe a sort of scientific precedence, not as the metropolitan, but the learned continent—earth's university. Europe alone is the adult continent, if there be one; and no other has in a strict sense a metropolis. The history of no other has so turned upon one pivot city as that of Europe has on Rome, nor is likely ever to do so. Some one says that "what a church is to a city, Palestine is (or may some day be) to the world;" but it is less disputable that what the marketplace is to a city, Europe is to the world—perhaps permanently. And what the tribunal is to the market-place, Rome has been to Europe, as long as Europe was growing. Observe, too, that in this special connection both our civilised time reckonings, "Old Style" and New, have come from Rome. Might we not also supersede the distinction of E. and W. longitude, by calling Rome 180°, and reckoning all round, from Behring to Behring, leaving the 0° as yet unmarked?

E. L. G.

[E. L. G.'s proposal has been already made by M. de Beaumont. See NATURE, vol. xix. p. 247.—ED.]

### Shakespeare's Colour Names

I FEAR you will think that the correspondence on this subject is becoming a mere criticism on Shakespeare's text, and therefore out of place in your columns, but I trust you will afford me space for a short rejoinder to Mr. Ingleby's letter (NATURE, vol. xix. p. 244).

I am obliged to him for pointing out that Sir T. Hamner had already suggested the substitution of "keen" for "green" in the passage from "Romeo and Juliet," Act iii. Sc. 5. This had escaped me, but I cannot agree with him that the alteration has been *rightly* rejected by subsequent commentators. I have not at present any opportunity of examining the eyes of any living eagles, but in opposition to Mr. Craig-Christie's evidence (NATURE, vol. xix. p. 221) I must point out that all our best